

EVOLUTION OF NERVES AND NERVO-SYSTEMS¹

III.

THE question, however, remains: Will this conductile function prove itself as tolerant towards section of the tissue as the contractile function has already proved itself to be? for, if so, any objection to the view that the passage of the *contractile* waves is due to vicarious action of rudimentary nerve-fibres will be removed. Briefly, the answer to this question is an affirmative; for I find it is quite as difficult to block the passage of stimulus waves by means of interposing cuts, as we have seen that it is to block the passage of contractile waves by the same means. For instance, here is an *Aurelia* (Fig. 5), the bell of which has been cut into the form of a continuous parallelogram of tissue, and then submitted to the tremendously severe form of section which is depicted. Yet on very gently stimulating any point in this expanse of tissue, as at the end *a*, a tentacular wave would course all the way along the margin, to *b*, thus showing that the wave of stimulation must have passed round and round the ends of all the intervening cuts. In the diagram the tentacular wave is represented as having traversed one-half of the whole distance from *a* to *b*, and near *b* there is represented a single remaining ganglion, (*g*). When, therefore, the tentacular wave reaches *g*, this ganglion will shortly afterwards discharge, so giving rise to a contractile wave, which will course back from *g* to *a* in the opposite direction to that which the stimulus wave had previously pursued.

And this, I am not afraid to say, is the most important observation, both to the physiologist and to the evolutionist, that has ever been made in the whole range of invertebrate physiology. For to the physiologist this observation proves that the distinguishing function of nerve, where it first appears upon the scene of life, is a function which admits of being performed vicariously, to almost any extent, by all parts of the same tissue-mass; while to the evolutionist the observation proves the existence of such a state of things as his theory of neurogenesis would lead him to expect. In such a symmetrically-formed animal as a Medusa, with all parts of the contractile sheet precisely resembling one another, we should expect the lines of discharge composing the hypothetical plexus to be very numerous, and all very much alike with respect to the degree of their evolution. For, as the symmetrical form of the disk does not require that any one set of lines should be used much more frequently than any other set, it follows from Mr. Spencer's theory that all the lines should more or less resemble one another as regards the extent of their differentiation.² That is to say, they should all be lines presenting about the same degree of resistance to the passage of a stimulus wave, and therefore it should become a matter of indifference, so to speak, through which particular set of lines such a wave takes its course.

There is still another class of facts which to my mind makes very strongly in favour of Mr. Spencer's theory.

¹ Abstract of a Lecture delivered at the Royal Institution on Friday evening, May 25, 1877. By George J. Romanes, M.A., F.L.S., &c. Continued from p. 271.

² Mr. Spencer himself observes, "The average equality of the forces to which their bodies (i.e., those of the Medusæ) are exposed all round is unfavourable to the formation of distinct muscles and a distinct nervous system." ("Psychology," vol. i. p. 522) Although this statement must now be modified so far as the ganglionic system of the Medusæ is concerned, I do not think that the anticipation which it embodies should on this account be deemed unwarrantable so far as it applies to other parts of the nervous system. For although it is true that a Medusa as a whole is "exposed all round" to an "average equality of forces," it is not true that the *excitable* portions of a Medusa are thus equally exposed. On the contrary, the margin of the excitable sheet which lines the cavity of the bell, occupies a much more exposed position than does any other part of that sheet; and whether or not this fact has anything to do with the development of the ganglia in the only part of the excitable sheet which is thus peculiarly situated, I think it is obvious that this part of a Medusa ought to be carefully excepted in the statement which I have quoted. With regard to all other parts of the excitable sheet, however, the statement is certainly correct; and it is only to such parts that the considerations in the text apply.—G. J. R.

Assuming, as I think we are now entitled to assume, that the contractile waves are not merely muscle waves, but depend for their passage on the progressive passage of the stimulus waves—assuming this, the following facts become facts of great significance. When the contractile waves in a spiral strip have become suddenly blocked by section, in the great majority of cases, such blocking will be permanent—even though the strip be continuously stimulated, whether artificially or by a single terminal ganglion, as represented in Fig. 4. But in the remaining cases, after a time that varies from a few minutes to a day or more, the obstruction is overcome, and the contractile waves pass forward with perfect freedom. Now, if I had time, I could prove that these facts are certainly not to be attributed to what physiologists term *shock*; and, therefore, it seems to me that only one hypothesis remains. What I have recently said about most of the lines of discharge in the supposed plexus being very much alike as regards the degree of their differentiation, does not, of course, mean that all the lines are *exactly* alike in this respect; for on *à priori* grounds such a state of things would be in the last degree improbable. Consequently, in conducting a spiral section, it must happen that at every snip the scissors cut through a number of lines of discharge presenting various degrees of differentiation; and, such being the case, the fact of the sudden and final blocking is presumably due to a well-differentiated line having been severed in a part of the tissue where no other line occurs of a sufficient degree of differentiation to conduct the stimulus forward. Now in most instances, as we should expect, the blocking so caused is permanent; for it is manifest that the formation of nervous channels, in the way suggested by Mr. Spencer, cannot proceed at so great a rate as to admit of *wholly* new lines of discharge being established during the life-time of a mutilated Medusa, i.e., during the course of a few days. Nevertheless, according to the hypothesis, some small percentage of cases might be expected to occur in which such blocking of the contractile waves would only be temporary. For some cases would almost certainly occur in which the relations of the highly differentiated line just destroyed to the more slightly differentiated lines in the neighbourhood of the section, would happen to be such that the more slightly differentiated lines would be very nearly, though not quite, able to act vicariously for the more highly differentiated line which has just been destroyed (see Fig. 4, where the deep line represents the well-differentiated line which has just been severed, and the dotted line the less-differentiated one which is still intact). The contractile waves, therefore, would in the first instance become suddenly blocked at the end of the strip. But the molecular, and with them the contractile, waves still continuing to pass quite up to the end of the strip, and being there always suddenly stopped, a rude conflict of molecular forces will thus set up in the area where these waves are impeded, and each of the forces concerned will seek for itself the line of least resistance. Hence, as the successive waves beat rhythmically on the area of obstruction, more or less of the molecular disturbance must every time be equalised through those lines of discharge which from the first have been almost sufficient to maintain the physiological continuity of the tissue. Therefore, according to the hypothesis, every wave that is blocked imposes on these particular lines of discharge a much higher degree of functional activity than they were ever before required to exercise; and this greater activity causing in its turn greater permeability, a point will sooner or later arrive at which these lines of discharge from having been *almost* become *quite* able to draft off sufficient molecular motion, or stimulating influence, to carry on the contractile waves beyond the area of previous blocking. In such instances, of course, we should expect to find, what I always observed to be the case, viz., that the first contractile waves which

pass the barriers are only very feeble, the next stronger, the next still stronger, and so on, according as the new passage becomes more and more permeable by use; until at last the contractile waves pour over the original barrier without any perceptible diminution of their force. In some cases, by exploring with graduated stimuli and needle-point terminals, I was able to ascertain the precise line through which this eruption of stimulating influence had taken place; so that altogether I think these facts tend very strongly to confirm Mr. Spencer's theory regard-

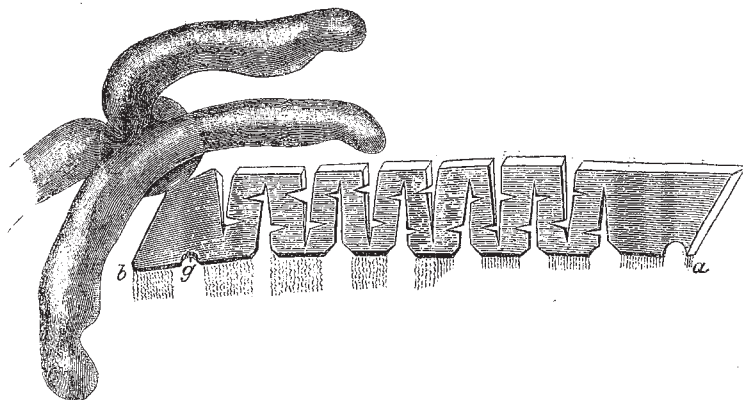


FIG. 5.

ing the genesis of nerves.¹ I will only add that if this interpretation of the facts is correct, we have in them a striking instance of the uniformity with which Nature works. A scientific theory concerning the evolution of nerves, which a year or two ago it seemed impossible to

¹ As additional proof that a wave of stimulation may pass over a barrier of the kind described in too small a quantity to start a wave of contraction beyond the barrier, I may mention the following facts:—In *Aurelia* the polypite is more sensitive to stimulation of the bell than is the bell-tissue itself; so that it is possible to stimulate the bell-tissue too gently to start a contractile wave in it, and yet strongly enough to cause writhing motions of response on the part of the polypite. Now, if by means of a spiral section of the bell the contractile waves have become blocked in the ribbon-shaped strip, it is sometimes possible, by strongly stimulating this strip, to cause the writhing motions of response on the part of the polypite—thus showing that although the contractile waves are blocked by the spiral section, the stimulus waves are able to pass forward with a strength sufficient to cause response in the polypite. I may here add that this fact of the contractile waves being sometimes wholly blocked by section before the stimulus waves are so, would appear to exclude, in the case of the Medusæ at all events, Kleinenberg's view as to the functions of primitive nerve and muscle being bleaded in the same tissue-elements. (See his work on *Hydra*.) I may also mention that in some cases I have observed that the establishing of a new line of physiological connection is a more gradual process than stated in the text. To show this, I may briefly quote one very instructive case. Seven marginal bodies having been removed, the eighth one continued to originate contractile waves, which coursed round the swimming-bell as us. I now made a radial cut half an inch on one side of the marginal body, and extending to the centre of the swimming-bell. The contractile waves were immediately blocked—thus showing, as did a somewhat similar experiment detailed in my first Royal Society paper (p. 293), "that the influence of the marginal body had previously been communicated to the swimming-bell from one side only." But in the case we are now considering, the discharges of the marginal body were still rendered apparent by very local contractions of a tissue area in the immediate vicinity of that body—the area, namely, which in the figure (Fig. 6) representing one end of the strip is marked B B. Exploration by stimulus now showed that general contractile waves could only be started outside the area B B. In somewhat more than half an hour after the operation (during which time the area B B continued to contract rhythmically), the ganglionic influence for the first time extended from the area B B to the rest of the strip—the contraction being therefore general. After this first eruption of contractile influence, there succeeded a period of about a minute, during which the area B B continued to contract independently as before. Then another eruption took place, followed by another period of restricted contraction, and so on. Next, these general contractions became progressively more and more frequent, and as the rhythm always continued the same, whether the contractions were local or general, the number of the latter became increased at the expense of that of the former. Thus, while at first there were twenty or thirty local contractions between every two general contractions, this proportion gradually fell to fifteen, ten, five, &c., till the numbers became equal, after which the balance began to incline in favour of the general contractions. Eventually the local contractions ceased altogether, and on now excising the marginal body and exploring by stimulus, I was able to localise very precisely the line through which physiological continuity had been established between B B and the rest of the contractile strip. This line was A C, as shown by the fact that while stimulation of any other part of the area B B was followed only by a local contraction at that area, stimulation of the line A C was always followed by a general contraction.—G. J. R.

verify, from the fact that it seemed as though the observations which would be required to verify it would need to extend over thousands of years—this theory is now, I believe, being verified by observations which need only extend over hours and minutes. The immensely protracted history of *nervo-genesis* upon this planet is thus probably reproduced in a greatly foreshortened manner in the facts which I have explained; and inconceivable as is the difference between these two histories of *nervo-genesis* in respect of their duration, it is nevertheless most probably in respect of their duration alone that these two histories differ.

I will now invite your attention to another species of Medusa, which is of a somewhat more highly evolved type than *Aurelia*, and which I have called *Tiaropsis indicans* (Fig. 7), in allusion to a highly interesting and important function which is displayed by its polypite. This function consists in that organ localising, with the utmost precision, any point of stimulation situated in the bell. For instance, if the bell be pricked with a needle at this point (a), the polypite immediately moves over and touches that point, as represented in the diagram. If immediately afterwards any other part of the bell be pricked, the polypite moves over to that part, and so on. Now this, you will perceive, is a highly remarkable function; for it proves that all parts of the bell must be pervaded by lines of discharge, every one of which is capable of conveying a separate stimulus to the polypite, and so of enabling the polypite always to determine which of the whole multitude is being stimulated. This localising function of the polypite, therefore, shows that the lines of discharge must be more differentiated in this species than they are in *Aurelia*; for it shows that vicarious action cannot be possible among them in so high a degree: every line of discharge must here have acquired a more specialised character, in order that the message which it conveys to the polypite when itself directly stimulated

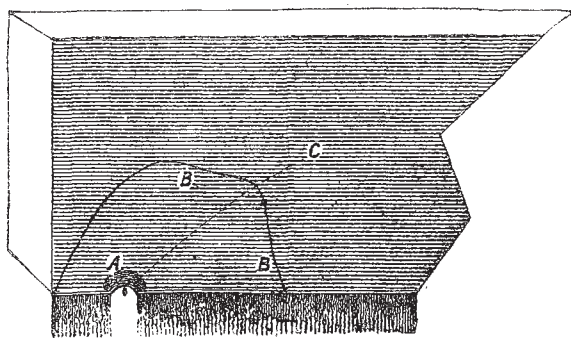


FIG. 6.

may not be confused with that which is conveyed by any other line.

Now it is easy to be wise after the event; but the state of things we here observe is just such a state of things as I think we should expect to constitute the next stage of *nervo-evolution*. It is no doubt a benefit to this Medusa that its polypite is able to localise a seat of stimulation in the bell; for the end of the polypite is provided with a stinging apparatus, and is besides the mouth of the animal. Consequently, when any living object touches the bell—whether it be an enemy or a creature serving as prey—it must alike be an advantage to the Medusa that its polypite is able to move over quickly to the right spot,

in the one case to sting away the enemy, and in the other to capture the prey. Hence I think that natural selection would probably tend to convert lines of discharge in promiscuous directions, into lines of discharge in definite directions—thus developing the function of localisation. At first, no doubt, this function would be performed only in a general and tentative manner (as, indeed, I have observed in the case of *Aurelia*); but gradually by the combined action and mutual reaction of use and survival of the fittest, this function would come to be performed with ever-increasing precision.¹

This, then, I conceive to be an important step in the evolution of nervous systems—foreshadowing as it does the principle of co-ordination among muscular movements, which in all the higher animals is effected by reflex mechanisms precisely resembling, as to their function, the primitive reflex mechanism we are considering. But now another point of interest arises. As Spencer's theory supposes a line of discharge to become more and more definite by use, if, for the maintenance of any particular function such as the one we are considering, a certain line of discharge habitually serves as a line of communication between two points of the animal tissues; it follows that this line will offer less resistance to the passage of a stimulus between these two points than would any other line in the organism. Consequently, so long as such a line remains intact, so long we should expect what we have seen to be the case, viz., that little or no vicarious action takes place between it and other lines. But let this line be severed, and let there be a number of closely adjacent lines, as there must be in this particular instance, and should we not expect, both from Spencer's theory and

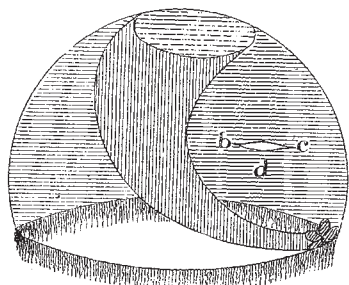


FIG. 7.—*Tiaropsis indicans*, slightly enlarged.

from our knowledge of *Aurelia*, that at some such grade of nervous evolution as *Tiaropsis* presents, the stimulus should be able to *escape* from the severed to the unsevered lines? And this I find to be the case. For if a small cut be introduced between the base of the polypite and the seat of injury in the bell, the polypite is no longer able to *localise* the seat of injury, although it still continues to perceive, so to speak, that injury is being applied *somewhere*. For instance, if a short cut be introduced as here represented at *b c*, and you prick the bell anywhere below the cut, as at *d*, the polypite, instead of immediately applying its extremity to the exact spot that is being stimulated, now actively dodges about first to one part and then to another part of the bell, as if seeking in vain for the offending body, which, however, it cannot succeed in finding. Now I explain this marked change in the behaviour of the polypite by supposing that the wave of stimulation in this case runs along the habitual line of discharge till it reaches the cut; and that being there no longer able to pursue this habitual line of least resistance, the wave of stimulation escapes into the adjacent lines, and so spreads all over the bell. Hence a number of conflicting messages are simultaneously delivered to the polypite, which therefore executes the random movements I have

described—each of these movements being presumably determined by the relative degree in which now one line and now another takes part in conveying the scattered stimulus.

And now for another expectation to be realised. We should expect that the higher degree of specialisation which in these lines of discharge prevents vicarious action so long as the lines are undivided, should have the effect of rendering such vicarious action as we have seen to ensue when the lines are divided, less easy than it is in *Aurelia*, where the specialisation of the lines being less pronounced, vicarious action among them is presumably more habitual. And such I find to be the case; for while in *Aurelia*, as we have seen, stimulus-waves continue to zig-zag round and round the ends of almost any number of overlapping cuts, in *Tiaropsis* two or three such cuts are sufficient to destroy, not only the localising, but also the random movements of the polypite—the latter then remaining passive, because the stimulus-waves are wholly blocked.

And lastly, before leaving the case of *Tiaropsis indicans*, I should like to mention the noteworthy fact, that although the polypite is able to perform the intricate ganglionic function of localising any seat of stimulation in the bell, no signs of ganglionic structure can be detected with the microscope. Moreover, a portion of any size that is removed from the polypite continues to perform the localising function in just the same way as does the entire organ. In other words, this localising function, which is so very efficiently performed by the polypite of this Medusa, and which, if anything resembling it occurred in the higher animals, would certainly have definite ganglia for its structural correlative, is here shared equally by all parts of the exceedingly tenuous excitable tissue that forms the outer surface of the organ. The case of the incipient ganglia of the polypite thus resembles that of the incipient nerves of the bell in this respect—that in both cases obvious signs of characteristic function are displayed before any corresponding signs of structure can be distinguished. Nerve-cells, therefore, no less than nerve-fibres, are thus shown to have their first beginnings in differentiations of protoplasmic substance which are too refined for the microscope to analyse.

There is one other species of Medusa about which I should like to say a very few words, because it presents a still higher grade of nervous evolution than *Tiaropsis*. This is *Sarsia* (Fig. 8), a Medusa in which the lines of discharge have in some places become so far differentiated as to admit of being actually seen, and are therefore entitled to be called nerves. All round the margin, and likewise along the course of the radial tubes, these, the earliest visible nerve-fibres in the animal kingdom, may be traced. And as we might anticipate, the advance of structure which is implied by an invisible "line of discharge" becoming a visible nerve-fibre, entails a corresponding advance of function. In the first place, the rate at which a stimulus travels seems to be much greater along these fully-evolved nerve-fibres than it is in the more rudimentary nerves or lines of discharge in *Aurelia*. In the next place, this greater differentiation of nerve-tissue renders the nervous connection between any two parts of the organism much more definite, and therefore vicarious action less promiscuous, than we have seen it to be in the other jelly-fishes; so that, for instance, a tentacular wave in this species may be blocked by a single short cut through the margin of the bell. Lastly, it is in this species that I was first able to perceive any unequivocal evidence of co-ordination among the marginal ganglia. In all the other species of Medusæ the marginal ganglia appear to act independently of one another; but in this species, where the marginal ganglia are first seen to be united by a visible nerve-fibre, they always act in concert. So much, indeed, is this the case, that the animal is able to steer itself in any required direction, as proved by the experiment which

¹ It may be here observed that Mr. Spencer, in his theory of *neuro-genesis*, expressly supplements his hypothesis as to the direct influence of use, with that as to the indirect influence of natural selection. (See "Biology," § 164.)—G. J. R.

I described last year, whereby individuals of this species were shown to have the power of following a moving beam of light round and round the vessel in which they were contained. I may also remark that individuals of this species present much more nervous energy than those of any other species of *Medusæ* which I have had the opportunity of observing.

I have now, ladies and gentlemen, communicated some of the points wherein my work has tended to elucidate the early stages in the evolution of nerves and nervous systems. And these are just the stages concerning which elucidation is most required. When once nerve-fibres and nerve-cells have been fully evolved and arranged in the form of simple reflex mechanisms, the subsequent history of their evolution into compound nervous systems is readily intelligible. The principles on which this higher evolution is effected are throughout the same, and result essentially in establishing ever more and more advanced

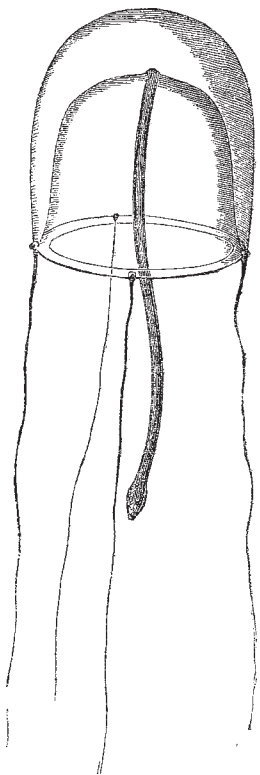


FIG. 8.—*Sarsia tubulosa*, \times three times.

degrees of integration. Compare, for instance, the nervous systems of an earth-worm, a centipede, an insect, and a spider; and observe the progressive fusion of ganglia which has taken place. The progressive centralisation which is thus effected is no doubt ultimately due to natural selection, if not exclusively, at any rate in large part; for this increasing consolidation of the reflex mechanisms must be of great benefit to the organisms which present it—serving as it does to render possible muscular movements ever more and more varied and combined. In the vertebrated series of animals the evolution of central nervous matter consists chiefly in adding to the size of ganglia by increasing the number of their ultimate nervous elements, nerve-cells and nerve-fibres. This progressive increase in the size of ganglia is especially remarkable in the case of the cerebral hemispheres. Now the cerebral hemispheres are the ganglia which we know to be the exclusive seat of the intellectual faculties; and their progressive increase in bulk as we ascend through

the animal series, is undoubtedly to be regarded as the structural correlative of that progressive advance of the intellectual powers which is so conspicuously apparent as we ascend from the lower animals to Man.

And now, in conclusion, I should like to observe, that even in this the highest product of nervous evolution—the supreme ganglia or cerebral hemispheres of Man—not only do we still encounter the same fundamental constituents of structure as we observe in all other ganglia; but the cells and fibres in the brain of a man do not differ in any marked degree from the cells and fibres in the ganglion of an *Aurelia*. There is, however, a prodigious difference in the product of their operation. When ordinary ganglion cells discharge their influence, the result is, as we have seen, a muscular contraction; but when cerebral cells discharge their influence, we of to-day can have no doubt that the result is a mental change. And although we freely acknowledge that we are here standing on the border-land of insoluble mystery, we are not afraid to assert with confidence, that in the amazing complexity of the brain's structure—amid those millions on millions of interlacing cells and fibres—we have the physical aspect of all those relations, which in their psychological aspect we know as thoughts and feelings. Do you think that this sounds like materialism? I am not here to-night to discuss that point; but I may observe in passing, that even were I able to tell you the particular cerebral elements which I now use in expressing this statement to you, I should be just as much or just as little on the way towards proving materialism, as I am when I tell you that a blow on the head produces insensibility. Science can never go further than common sense in proving any necessary connection to subsist between mind and matter; for all that science can ever do is to ascertain numerous details with regard to such connection as undoubtedly does exist, and which, as a matter of daily experience, common-sense has already and completely recognised. However, materialism or no materialism, it is manifest that the facts being what they are, Mr. Spencer's theory as to the genesis of nerves must not be allowed to stop short just where its presence is most required. As we have seen that the cerebral hemispheres of man resemble all other ganglia in structure, we cannot hesitate in concluding that if Mr. Spencer's theory is valid in explaining the genesis of nerves in general, it can be no less valid in explaining the genesis of these supreme ganglia in particular. And as we have every reason to believe that the functional operations of these supreme ganglia are inseparably associated with our thoughts and feelings, we are driven to the yet further conclusion, that if Mr. Spencer's theory is of any validity at all, our possible as well as our actual thoughts and feelings are determined by the strictly physical conditions under which molecular waves of stimulation course through the structure of the brain. So that in this Spencerian hypothesis of lines of discharge becoming more and more definite by use, we have a physical explanation, which is perhaps as full and as complete as such an explanation can ever be, of the genesis of mind. From the time that intelligence first dawned upon the scene of life, whenever a new relation had to be established in the region of mind, it could only be so established in virtue of some new line of discharge being excavated through the substance of the brain. The more often this relation had to be repeated in the mind, the more often would this discharge require to take place in the brain, and so the more easy would every repetition of the process become; until at last the line of discharge grows into a nerve-fibre, and becomes the inherited property of the race. Thus it is, according to the theory, that there is always a precise proportion between the constancy with which any relations have been joined together during the history of intelligence, and the difficulty which intelligence now experiences in trying to conceive of such relations as

disjoined. Thus it is that, even during the history of an individual intelligence, "practice makes perfect," by frequently repeating the needful stimulations along the same lines of cerebral discharge—so rendering the latter even more and more permeable by use. Thus it is that a child learns its lessons by frequently repeating them; and thus it is that all our knowledge is accumulated. In a word, if, as has been truly said, "man is a bundle of habits," we have in Mr. Spencer's theory of *nervo-genesis* a physical explanation of the fact. And forasmuch as it is upon this theory that Mr. Spencer may be said to found that great monument of modern thought—his "Principles of Psychology," I cannot but feel that one of the most important bearings which my work on the *Medusæ* has had, is that of supplying facts which tend to substantiate this theory—and this at a time when it seemed as though the theory could never have other than *à priori* considerations for its support. But if my interpretation of these facts is correct, this important theory is now receiving inductive verification from a most unexpected source. At first sight no two organic structures could well seem to have less in common than the swimming-bell of a *Medusa* and the brain of a Man; nor could anything seem more unlikely than that a great psychological theory should derive support from the study of polypes, where the very existence of a nervous system has only just been discovered. But here again, I believe, we may discern the uniformity of Nature; and while watching the passage of the waves of stimulation in the contractile strips of *Aurelia*—now passing freely, now stopped by an excess of resistance, and now again forcing a passage,—I have felt that I was probably witnessing, on the lowest plain of *nervo-genesis*, that very same play and counter-play of forces, which, on the highest plain of *nervo-genesis*, invariably accompanies, if it does not actually cause, the most intricate reasoning of a Newton, the most sublime emotion of a Shakespeare, the most imperious will of a Napoleon, and the most transforming thought of a Darwin.¹

ATOMS AND EQUIVALENTS

IN the *Comptes Rendus* for the month of May and June there is a series of communications by Messrs. Wurtz and Berthelot containing a discussion of their respective views as to whether chemical changes should be expressed by elements in equivalent proportions or whether the more modern system of atomic weights should be employed.

In the first communication, which is made by M. Wurtz, he remarks on the discrepancy evidently existing between his idea and that of M. Deville, "who has criticised him in a former number of the journal," on the law of volumes of Gay-Lussac. He advances in support of the atomic argument, that free hydrogen may be regarded as a combination of two atoms of hydrogen, the peculiar reaction of hydrochloric acid on hydride of copper and in the case of oxygen with oxygen the reactions discovered by Thenard and Brodie of peroxide of hydrogen on certain oxides. He maintains that the molecular conceptions with regard to bodies in the free state are further upheld, in the case of nitrogen by the formation of nitro and

dinitro compounds, and in the case of carbon by the consideration of organic chemistry when examined according to the theory of Kekulé of the grouping of several carbon atoms in the same molecule. After discussing the law of Gay-Lussac as applied to the gaseous compounds of hydrogen with chlorine, oxygen, and nitrogen, he remarks that what results from the previous discussions on this matter, is, that the system of expressing chemical reactions by equivalents which prevailed about 1840 over the atomic notation of Berzelius, has not taken into proper account the discoveries of Gay-Lussac on the combination of gases with each other; and consequently, that the maintenance of this principle in the discussion of chemical phenomena would cause a serious obstacle to the advancement of the science.

M. Berthelot on behalf of those who, like himself, retain the method of writing chemical changes by equivalents, as opposed to the atomic notation, in replying to this first communication of M. Wurtz, states that he does not think the matter to have the same importance which the latter seems to attach to it. He considers that the progress of chemical science is not entirely subordinate to a change of notation which does not strike at the foundation of the science as it had done a hundred years ago to the pneumatic chemistry of Lavoisier. He thinks that at the present day the truths are so general that all the laws may be expressed to a certain extent by both languages with equal clearness and precision. With regard to the view put forward by Wurtz, that bodies in the free state are composed of two atoms, and in support of which view he has mentioned the reactions of hydrochloric acid on hydride of copper, and peroxide of hydrogen on oxide of silver, $\text{Cu}_2\text{H} + \text{HCl} = \text{CuCl}_2 + \text{HH}$, and $\text{Ag}_2\text{O} + \text{H}_2\text{O}_2 = \text{Ag}_2 + \text{H}_2\text{O} + \text{OO}$. M. Berthelot deems the explanation given by M. Wurtz mere assumption, without sufficient proof, tending to prevent a true understanding of the real cause of the reaction. He also considers that the true explanation might be found in and explained by certain thermal considerations.

M. Berthelot passes next to a criticism of the atomic method of expressing the reactions of certain metallic salts with each other, and complains of the doubling of the equivalents of certain bodies, such as CaCl_2 , which he thinks makes an unnecessary complication in the expression of the reactions, and gives as an instance the reaction of certain nitrates with chlorides. By the system of equivalents, he maintains they might be expressed by one reaction— $\text{MNO}_3 + \text{M}'\text{Cl} = \text{MCl} + \text{M}'\text{NO}_3$ —but that by the atomic notation four different and distinct reactions are necessary to express their decomposition.

M. Berthelot then alludes to the confusion he thinks has arisen between the words law and hypothesis, in the acceptance of Avogadro's law. In this case he maintains that Avogadro and Ampère have enunciated, not a law, but an hypothesis, in saying, "All gases contain the same number of molecules in the same volume," having, in reality, nothing by which to conceive the idea of a molecule. On the other hand, he thinks the proposition, "The densities of gases or vapours are proportional to their equivalents," being deduced from two orders of properties observable by experiment, may be regarded as a true law. Partisans of the atomic notation have, he considers, substituted for this the proposition, "Molecules of simple gases contain the same number of atoms," and he complains that they thus introduce two hypothetical notions, that of the molecule, and that of the atom. On the other hand, supporters of the system of equivalents say, "Equivalent weights of simple or compound bodies occupy the same volumes;" or the volumes are to each other in the simple ratios, 1, 2, 3, 4, &c., thus:—

1	equivalent of oxygen occupies 1 volume.
1	" Cl, H, or Hg occupies 2 volumes
1	" HCl occupies 4 volumes, &c.

M. Wurtz, on the other hand, replaces the above by the

¹ Throughout the lecture of which the above is a pretty full abstract, I have associated Mr. Herbert Spencer's theory of *nervo-genesis* with his name exclusively. To avoid misapprehension, therefore, I append this note to state that I am not ignorant of the fact that the theory in question has occurred to other thinkers as well as to the great English philosopher. Moreover, I am quite aware that even if this theory of *nervo-genesis* had never been enunciated *à priori* by any speculative thinker, some such theory would certainly have been devised *à posteriori* by any working physiologist of moderate capacity who might first happen to observe such facts as are above detailed. But considering that Mr. Spencer elaborated the theory deductively, and that he did so in a much more thorough and painstaking manner than had ever been done before, considering, too, that he has given the theory so elaborated such a prominent place in his system of objective psychology, I have not hesitated to describe this theory as being pre-eminently a product of his authorship.—G. J. R.